



THE ROCKEFELLER UNIVERSITY

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NEW YORK, NY 10021

January 5, 1987

JOSHUA LEDERBERG

PRESIDENT

Dr. Stephen M. Stigler
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Dear Steve:

Thank you for your enthusiastic comments about our "Post-mature" article. The copy editing was atrocious. The print version, which came out in great haste, ignored several corrections that Harriet and I had wired. I had sent John Maddox the manuscripts in early June. They waffled until November, and then suddenly decided that it needed to get into print whilst 1986 still permitted a headline of an anniversary. They also demanded rather drastic cutting; and while this had some benefits, some other values were also lost in the process.

I suppose there must be some meaning to Darwinian vs darwinian. Perhaps darwinian reflects the extent to which Darwinian theory today is a far remove from anything that Charles Darwin could have understood. E.G. he was ignorant of Mendel! On the other hand, Mendelism today means pretty much just what Gregor Mendel wrote down in 1865.

On the question of high risk research, I can assure you that I was quite aware of the odds, (possibly even more pessimistic than a retrospective accounting would justify, apart from the fact that the experiment did work!) What I am more often challenged about was whether I understood the value of the stakes; and there again I can assure you that I did. This is part of a running dispute about whether anyone "really understood" the significance of Avery's findings in 1944. That seems to be a regionally differentiated matter: there is no doubt what the current of excitement was at Columbia! Also as Harriet has taken pains to stress, what was there to lose?

In addition -- not brought out in the article -- was the certainty (and my conviction about it) of making discoveries along the way, almost no matter what one targets, if one keeps one's eyes open. This had already materialized in our unsuccess-

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ful attempts to transform *Neurospora leucineless* to leucine-independent: but they did result in a discovery, the first demonstration of reverse mutations in *Neurospora* and the method for seeking them selectively. For the rest of my career that paradigm has never failed; and I have repeatedly made many more unanticipated discoveries than any I had planned for. That perspective informs my present feeling of revulsion about the project system of supporting research (as opposed to the identification of worthy individual scientists). But perhaps my own career is at one extreme of exploration (Koestler called it sleep-walking) and that may be atypical of the great bulk of normal science.

Forgive me for going on at this length; but I thought it a good idea to put these notions on record while they were still fresh in mind.

I very much sympathize with your grievance about Nature not publishing your letter. The stiffness of scientific communication is one of many reasons I have been enthusiastically backing Gene Garfield in his new newspaper of science, "The Scientist". We are trying to start a column of feedback, which will allow for commentary that original journals of record refuse to publish. Would you care to submit your letter for publication in The Scientist? When we get around to electronic, multi-tier, publication we may have still less clumsy ways of maintaining the integrity of the scientific dialectic.

All the best,

Yours sincerely,



Joshua Lederberg

cc: Harriet Zuckerman

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